Reply to the Comment, arXiv:0810.3243v1 by B. Geyer, G. L. Klimchitskaya, U. Mohideen, and V. M. Mostepanenko

L. P. Pitaevskii
CNR INFM-BEC, Department of Physics, University of Trento, I-38100 Povo, Trento, Italy;
Kapitza Institute for Physical Problems, Kosygina 2, 119334 Moscow, Russia.

It is shown that criticism of paper [2] by the authors of the Comment [1] is wrong and and that their main arguments are in contradiction with established concepts of statistical physics.

PACS numbers: 34.35.+a,42.50.Nn,12.20.-m

1. The main point of the Comment is that the theory developed in [2] "leads to the violation of Nernst's theorem" for materials where "the concentration of charge carriers, n, does not go to zero when temperature vanishes, but the conductivity goes to zero due to vanishing mobility μ".

I believe that this statement is a result of a pure misunderstanding. The materials under discussion are amorphous glass-like *disordered* bodies. Conductivity goes to zero with T in such materials due to localization of the charge carriers just because of the disorder. The point is that Nernst's theorem *is not valid* for these disordered bodies. It is well-known that they have a big finite entropy at zero temperature. Localized carriers also contribute to this residual entropy and the calculation of a small correction to its value due to the Casimir-Polder interaction scarcely has a physical meaning. Of course, the existence of disordered bodies at $T = 0$ itself does not contradict quantum statistical mechanics. They are simply not at an equilibrium state at low temperatures due to a very long relaxation time. Particularly they are not in their ground state at $T = 0$. The criticism of an application of the original Lifshitz theory to these disordered bodies on the basis of Nernst's theorem in [3] is also wrong for the same reason.

2. The authors state: "Physically, the theory of [2] includes the effect of screening, i.e., nonzero gradients of n. This situation is out of thermal equilibrium which is the basic applicability condition of the formalism of [2]".

This statement is obviously wrong and odd. It is well-known that the Boltzmann distribution in the electric field, which was used in [2] for describing screening, is an *equilibrium* distribution. Actually, in equilibrium, the diffusion current due to the gradient of n is compensated by the mobility of carriers due to an electric field. This compensation results in Einstein's relation between the coefficients of diffusion and mobility.

3. The authors say that papers [2, 4, 5] violate "the Nernst theorem for metals with perfect crystal lattices". This severe statement is based, however, on the paper [6], which is certainly wrong, because the authors used the *normal* skin effect theory for metals with perfect crystal lattices at $T \to 0$, while it is well-known that in this situation one must use the *anomalous* skin effect theory (see [7]). Consequently it is impossible to make any conclusions on the basis of this paper.

4. The authors object to my statement that "it is difficult to estimate the number of ions which are effective in mobility and screening" on the basis that n "can be obtained by the method" presented in [8]. However, the results of the paper [8] confirm my point of view. At given number of impurities atoms $n_{Na} \sim 2 \times 10^{15}\mathrm{cm}^{-3}$ and at $T = 433\mathrm{K}$, the number of carriers n in fused silica changes from $3\times10^{12}\mathrm{cm}^{-3}$ to $2\times10^{8}\mathrm{cm}^{-3}$ depending of the water content. Furthermore, the measured temperature dependence of n is in strong contradiction with the authors' assumptions. When temperature decreases from 473K to 433K, n *decreases* from $6 \times 10^{13}\mathrm{cm}^{-3}$ to $3 \times 10^{12}\mathrm{cm}^{-3}$ and the authors of [8] explicitly conclude: "The change in conductivity nearly scales with the change of charge carrier concentration suggesting that *the mobility remains nearly independent of temperature*"[9].

5. I do not have enough information about details of the experiment and calculations [10] to discuss their accuracy. In general, if an experiment and a theory do not agree, it does not always mean that the theory is wrong.

The authors believe that my statement that for the relaxation time of the order of 917 hours "the carriers mobility can hardly be important in any experiments" agrees with their prescription "that for dielectrics the dc conductivity should be disregarded". However authors of [10] disregard the dc conductivity only in absence of light and take it into account in the presence of light, without any foundations for this difference. In my opinion this procedure is not consistent. In fact, I wanted only to stress that the screening plays no role if the relaxation time is larger than the duration of an experiment. However, I see no reason not to take into account the conductivity if equilibrium is reached.

In conclusion I would like to emphasize that I do not think that all problems of the thermal Van der Waals forces are clear both from the theoretical and experimental points of view. However, their solution cannot be based on discarding established concepts of statistical physics.

I thank M. Tomosawa for useful discussion properties of fused silica.

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[5] V. B. Svetovoy, Phys. Rev. Lett. **101**, 163603 (2008).
[6] V. B. Bezerra, G. L. Klimchitskaya, V. M. Mostepanenko, and C. Romero, Phys. Rev. A **69**, 022119 (2004).
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[9] I do not know if it is possible to extrapolate this dependence to $T = 0$. As it follows from item 1, it makes no difference for our discussion.
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